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THE METEOROLOGICAL MAGAZINE

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A REPORT ON A DISCUSSION "INTERACTION BETWEEN THE ATMOSPHERE AND THE OCEANS"

By L. JACOBS

A special all-day meeting took place at the Meteorological Office, Bracknell, on Tuesday, 16 January 1962.

The Chairman, Professor Sheppard, opened the meeting by explaining that the present discussion arose from a suggestion by the Meteorological Committee which was pursued by Dr. Sutcliffe. It had been agreed that it would be most useful to have a discussion between oceanographers and meteorologists on work currently in progress or projected in the United Kingdom on the interaction between the atmosphere and the oceans. The present meeting was a combined one of the Meteorological Research Committee and its two Sub-Committees, I and II, and Professor Sheppard welcomed the oceanographers who attended it.

Storm surges and meteorological disturbances of tides

Dr. J. R. Rossiter of the University of Liverpool Tidal Institute opened the discussion by giving an account of "Storm surges and meteorological disturbances of tides". He explained that even small meteorological disturbances could have economic importance in reducing the value of tidal predictions and instanced the Great Australian Bight where tide timing errors of up to two hours could occur through this cause. The larger storm surges were a hazard to property and even to life. The present subject was thus a very practical one and indeed a topical one as only the previous weekend there had been storm surges over the western coasts of the British Isles and in the English Channel.

Up to recent years, the work on storm surges, which had started at the Tidal Institute in 1928, had depended entirely on the semi-empirical method of obtaining appropriate equations, including meteorological variables, from a study of the basic hydrodynamical equations, and then using statistical methods, with carefully selected surges, to determine the empirical constants. Results had been reasonable—some hind-casting experiments had yielded correlation coefficients between observed and computed surges of as much as 0.95. The Admiralty Flood Warning Organisation adjacent to the forecasting office at Bracknell does in fact, by such methods, combine the data from the network of reporting tide-gauges with meteorological forecasts for the North Sea area to

issue warnings when dangerous levels are likely to be reached along the east coast. Such simple prediction methods fail, however, when assumptions implicit in the basic theory, of which the two main ones are of equilibrium conditions and of the astronomical tide being independent of the meteorological surge and vice versa, are only too often violated in our coastal waters. The assumption of equilibrium conditions was reasonably valid for the North Sea, but marked resonance effects can occur on the west coast, particularly in the southern Irish Sea, when secondary depressions cross the area—this was illustrated by a slide showing the synoptic situation on 18 October 1957. The second main assumption is valid only if the second- and higher-order terms in the hydrodynamical equation for the water movement can be ignored, and this is certainly not true in shallow water where, considering idealized one-dimensional flow, the non-linear terms involving friction become important.

Before going on to consider the modern approaches to direct solution of the hydrodynamical equations, Dr. Rossiter emphasized that, as this was such a difficult problem, the present empirical-cum-statistical method will continue to be of considerable service in many branches of surge research and forecasting. Models used in an attempt to solve the equations were of three types, small-scale physical, as at the Hydraulics Research Station, Wallingford; electronic, as at the National Institute of Oceanography and in Holland; and mathematical as at his Institute (and a number of other institutes). All models can incorporate bottom friction, wind stress on the sea surface and geostrophic forces; the mathematical model involves the use of a large electronic computer.

Dr. Rossiter then gave details of the one-dimensional mathematical model in use at his Institute; the main problems were the physical ones of assuming and ascertaining an average value of horizontal water velocity over the depth, and of trying out various values of bottom friction, and the mathematical ones of the representation of the shape of a channel, the boundary conditions at irregular coasts and the ideal size of the computation mesh. He made particular reference to his experience with a mathematical model of the River Thames; the present model would not yield the right answer without considering the interaction already present at the sea end which led to the formidable task of dealing with a two-dimensional model, necessary anyhow if wide bodies of water—with geostrophic force present—are to be considered.

In pointing out the need for a more complete mathematical theory, amenable to numerical solution, Dr. Rossiter mentioned that recent theoretical work, supported by small-scale tank observations, showed that surface wave phenomena, normally ignored since their period is assumed to be below that of the surge, can appreciably affect the surge height, so that this factor has also to be considered. He concluded by emphasizing that any method of surge forecasting is very largely dependent upon a reliable forecast of the synoptic situation, and in particular of the detailed wind field over the sea, for at least twelve hours ahead; the meteorologist could also help to ascertain the precise law which controls the response of the sea to wind stress.

Dr. Deacon of the National Institute of Oceanography said that the scope of the problem is enormous. For very slow changes the sea acts like an inverted barometer and in the open ocean a rise in atmospheric pressure of one millibar depresses the sea level by one centimetre. In an estuary the response is less. For more rapid changes the response is likely to be greater than one to one, because

of the partial resonances with natural periods of bodies of water on the coastal shelves or in bays or harbours, or because the pressure disturbance travels at about the speed of a free wave in the shallower coastal waters. This seems to be the main cause of the disastrous hurricane surges on the east coast of the United States of America, of the smaller ones which occur from time to time on our own west coast, and of the small unexpected surges which occasionally cause some confusion on our south coast beaches. In the North Sea the piling up of water trapped in a constricted area in front of the wind seems more important, but anti-clockwise movement of the surge down the east coast of Britain and northwards along the coast of the continent shows an important dynamic element.

Mr. Crease of the National Institute of Oceanography, in commenting that Dr. Rossiter had shown that the semi-empirical methods of prediction of storm surges are capable of high accuracy over a length of time sufficient to be of immediate practical use, emphasized that the relatively infrequent occurrence of large surges does, however, hinder the investigation of the dynamic coupling between atmosphere and ocean and, at the same time, the treatment of solitary waves is somewhat more awkward mathematically than that of periodic disturbances. There is the alternative approach of studying continuous records of sea level and atmospheric fluctuations by spectral analysis. By analysis of records from more than one station much should be learnt of the direction and rate of growth of surges in different parts of the spectrum.

Sir Graham Sutton pointed out that in the meteorological problem, equations of motion had to be filtered to eliminate sound and gravity waves. This was done by introducing the hydrostatic equation and the geostrophic approximation in the right places. He wondered whether this had been considered for the oceanographic equations. *Dr. Rossiter* said that at a conference at Hamburg last year there had been a discussion of the use of finite difference equations. Stability had not been discussed in much detail: some speakers had mentioned the question of smoothing, but this had been rather frowned on. He referred to the diagrams he had shown as illustrating a smoothing process.

Mr. Sawyer was surprised to see that the oceanographers were using the basic equations, as the meteorologist had considered it far simpler to introduce one function, e.g. a stream functions in the analysis. *Professor T. V. Davies* of the University of Wales, Aberystwyth, wondered what the typical wavelength of the storm surge was. This length should give a guide to the grid spacing required. *Mr. Crease* pointed out that the geostrophic approximation was not appropriate as in this case the oceanographer was concerned with the inertia-gravity waves. The water is treated as incompressible, so sound waves do not confuse the picture.

Dr. Rossiter, replying to these points, stated that the non-linear terms of importance in estuaries and shallow seas made the use of only one variable extremely difficult mathematically. The period of the North Sea surges varied from 18 to 36 hours and the wavelength is therefore comparable with the length of the North Sea.

Dr. Hunt of Imperial College asked if it had yet been possible to compare results from the Tidal Institute model of the Thames with those obtained on the Port of London Authority model operated by the Hydraulic Research Station,

Wallingford. It is presumably easier to specify independent tide and current curves at the seaward end of the mathematical model than in the hydraulic. The latter, however, has the advantage of permitting more realistic distributions of velocity with depth. Regarding *Dr. Rossiter's* comments on the necessity of including both horizontal components of velocity when incorporating the region seaward of Southend, surely predictions for the one-dimensional region upstream of Tilbury, say, would not be sensitive to the detailed structure of the flow in this area so that a reasonable one-dimensional average might be used. *Dr. Rossiter* replied that a comparison of the physical and mathematical models will be made and will be useful. He hoped that the mathematical model will be of some practical importance; the model of the Thames for storm surge research is more useful if used as an adjunct to the model of the North Sea, where the two-dimensional analysis would be necessary.

Dr. Sutcliffe emphasized that it was essential for the meteorologists and the oceanographers who were considering similar mathematical problems, involving the equations of motion, to work closely together. He and his staff would be pleased to welcome any oceanographers who would care to visit this Headquarters for detailed discussion on this problem. *Cdr. Synnott* said that in his experience in the Flood Warning Organization (based at the Meteorological Office, Bracknell) the path of the depression was an important factor as with different paths the behaviour of storm surges was different, even if the winds were the same. He pointed out that negative surges were of particular importance, e.g. to the Port of London Authority; on the morning of the discussion the level of the Thames had been 5 feet lower than that predicted.

Mr. Sawyer stated that the details of wind structure were not known near the centres of depressions over the sea since information was limited to that provided by anemometers on the coasts; topography was important, e.g. near the Norwegian mountains. *Dr. Rossiter* said this point had been discussed at the Hamburg conference, when considering the North Sea. Estimates were made of the geostrophic wind but curvature of the air trajectory was not taken into account. When the latter was considered there was a negligible improvement to the result. He again emphasized that knowledge of winds near depressions over the sea was extremely important in this work.

Recent developments in the theory of wave generation by wind

Dr. O. M. Phillips of the Department of Applied Mathematics and Theoretical Physics, University of Cambridge introduced his topic "Recent developments in the theory of wave generation by wind" by stating that his aim was to present some of the physical ideas that had emerged in the last few years concerning this problem. There were two important mechanisms at work, resonance between the surface wave modes and the advected surface pressure fluctuations associated with the turbulent wind blowing over the water, and the gradual instability caused by the airflow over a surface already disturbed. Phillips[1] had discussed the resonance effect and had shown that the time scale of the fluctuations of a given wave-number is a maximum when the wind speed in the direction of wave propagation is just equal to the phase speed of free surface waves of the same wave-number; if this condition for resonance is satisfied there is maximum energy transfer from wind and waves.

Shortly after this paper was published, Miles[2] showed that if an airstream with a prescribed velocity profile blows over a water surface, then any small disturbance is dynamically unstable, but the amplification rate is very slow. The amplification factor in Miles's theory depends on the shape of the wind velocity profile and on the ratio of the wind component U along the direction of travel of the waves to the phase velocity c of the waves.

The Miles instability mechanism gives a rate of growth proportional to the magnitude of the disturbance already present so that the wave height is an exponential function of time. The resonance mechanism is independent of the waves already existing, and provides a linear rate of growth of wave energy. If the wind begins to blow over an initially calm sea the resonance from the atmospheric pressure fluctuations will give rise to a linear rate of growth which will, however, change to an exponential rate as the waves become large enough for the instability to take effect. However, for waves travelling at about the same speed as the wind, the resonance mechanism is operating at its best and the amplification rate of the instability is very slow. Recently Miles[3] has given the combined effect an analytical form. An interesting result from the analysis is that if the waves start from rest then, for a given frequency and wind profile, the time elapsed until the transition between resonant and unstable growth is independent of the magnitude of the atmospheric pressure fluctuations. Of course the amplitude of the wave motion is proportional to the pressure fluctuations, but the transition time is a function only of the shape of the velocity profile and the ratio U/c.

Phillips[4], assuming initial conditions of rest, calculated this transition duration as a function of the wind profile and U/c; with a wave disturbance already existing, when the wind begins to blow, the time required for transition was decreased.

This summary of the present state of the theory showed that there were a number of important questions outstanding. Miles' [3] theory assumes a simple addition of the effects due to atmospheric turbulence and the airflow over the wavy surface but there may well be some interaction; he also assumes a logarithmic wind profile which Stewart[5] certainly questions—and Dr. Phillips himself knew of many wind profiles over the sea which were certainly not logarithmic. There is the added point that when the transition duration is attained, the wave amplitudes may already be so large that the amplification factor calculated, assuming infinitesimal amplitude disturbances, may be in serious error. In spite of these questions a number of predictions from the theory had been borne out experimentally, as explained later by Dr. Phillips.

Dr. Phillips went on to point out that the wave growth by resonance and instability was limited by the waves breaking and forming white-caps because the particle accelerations cannot exceed g; at this breaking stage the energy is lost from the wave system to turbulence in the water. The mathematics of the process was discussed by Phillips[6] and expressions for the frequency spectrum and the two-dimensional wave-number spectrum were obtained for the limiting equilibrium or saturation state when breaking of the waves occurred. There had been good experimental support for these relations by measurements by Burling[7] taken on Staines Reservoir, Middlesex; on the open ocean as a result of the Stereo-Wave Observation Project[8] and by the National Institute of Oceanography group (Longuet-Higgins, Cartwright and Smith[9]) and by

small-scale experiments by Cox[10] and Hicks[11]. Dr. Phillips went on to consider the implications of these theories to the observation of ocean waves, to what extent the predictions are borne out by present measurements and what new observations might be made to provide further tests and further insight into the subject. He showed how one test of the theory was to check the identification between the frequency at which the steep forward face of the frequency spectrum is found at a particular duration and the frequency undergoing transition —when the wave amplitude grows rapidly. Phillips and Katz[12] had examined a number of observational spectra and found, as was expected, that the transition occurs not later than the theoretical values given by the Phillips'[4] curve. This comparison was promising but hardly convincing since it meant attributing the premature transition to a more rapid rate of growth, to the existence of an initial wave state of low energy which, though undoubtedly present, was not measured.

However, a further prediction from the theory provides a more critical test. This is that the transition occurs for a given frequency first for the components travelling in the direction of the wind, so that the observed frequency of the steep forward face of the spectrum is strictly the transition frequency for components travelling in the wind direction. Components with the same frequency but travelling at an angle to the wind will not yet have undergone transition, so that the directional distribution of frequency components near the spectral peak should be strongly oriented towards the wind direction. At a slightly higher frequency, transition will have occurred over a range of angles and the directional distribution should be much broader. Some of the results of Lonquet-Higgins, Cartwright and Smith[9] suggest this is so, as do casual observations at very short fetch and duration but definitive measurements have yet to be made. If the fetch and duration are large an appreciable part of the wave energy may be contained by components travelling at about the same speed as the wind, when, as shown earlier, the resonance mechanism is predominant. The wave energy should thus show directional maxima at angles $\pm \cos^{-1} (c/U)$ to the wind. The Stereo-Wave Observations Project[8] results show this expected bimodal distribution. However, the meteorological conditions were not well defined and there is some uncertainty in the wind field. Further evidence for the existence of directional maxima is given by the Longuet-Higgins *et alii*[9] experiments on a pitch and roll buoy.

Dr. Phillips also mentioned that another verification of the theory was to test the accuracy of a relation between the mean square surface displacement and the dominant frequency of the wave field, and this had in fact been discovered empirically by Hicks[11] in his observations of wind waves on a pond.

Mr. Cartwright of the National Institute of Oceanography emphasized the importance of this theoretical work on wave generation by Phillips and Miles, which gives a completely new look to the problem of practical wave forecasting in terms of meteorological data. It was hoped that these theories would eventually form a basis for forecasting methods which will supersede the crude existing methods with their many inconsistencies and failures. The National Institute of Oceanography is working on research in interpreting ocean waves in terms of these theories, but unfortunately it is a very difficult and slow process; the need is to measure directional spectra of waves in the open ocean

and this involves complicated instruments recording many wave characteristics simultaneously, which can only be handled by experts, and large quantities of data to be processed by electronic computer. Sufficiently simple meteorological conditions for direct testing of theoretical ideas are rare and too much time cannot be demanded of those on board a ship, or of skilled technicians, in order to wait for such conditions. The final fruits of this pioneer work of Phillips and Miles will come only after a long period of research, and greater effort could be applied to this research with advantage.

Mr. Lumb pointed out that Darbyshire[13] had stated, in a recent paper, that a fully developed sea disturbance occurred after a period of 12 hours. He also mentioned that there was some theoretical basis for the logarithmic wind profile over the sea, but the sea is often colder than the air and there are many occasions when a logarithmic profile is not applicable.

The Chairman mentioned the long-term project being operated by Imperial College at Lough Neagh in Northern Ireland; this work had shown that there were many occasions when a non-logarithmic wind profile occurred. The height of observation in these experiments was over the range one to 16 metres and with wave amplitude up to one metre or more it was not possible to measure the wind profile below one metre. Consequently it was very difficult to test any assumption about the nature of the flow at levels below that at which the wind speed was less than the wave speed, as referred to by Dr. Phillips. Burling's[7] work on Staines Reservoir involved a fetch of the wind up to one kilometre; in the Lough Neagh work there are fetches up to 25 kilometres.

Sir Graham Sutton drew attention to a similarity between one of the equations in Dr. Phillips' detailed presentation (involving the ratio of the second and first differential of velocity) and von Karman's definition of the mixing length in turbulence. He wondered what the relationship was to the normal turbulence theory in a sheared fluid. *Dr. Phillips* said this point had not so far been considered. He agreed with Sir Graham Sutton that a large value of this ratio meant a small mixing length and hence fine-grained turbulence. *Mr. Charnock* of SACLANT Research Centre, La Spezia, Italy emphasized that in any observational programme the structure of the wind field would need to be known at least as well as that of the wave field. Laboratory experiments could perhaps provide useful indications.

Exchange of energy across the ocean-atmosphere interface

Mr. D. H. Johnson, in opening the afternoon's proceedings by an account of the "Exchange of energy across the ocean-atmosphere interface", began by expressing the keen interest of the Climatological Research Branch of the Meteorological Office in this subject. It was well known that radiative processes by themselves result in a net atmospheric heat loss. This heat loss is made good by the turbulent transfer of energy from the earth's surface. Calculations have shown that much of this energy is transferred by evaporation taking place over the sea. Thus the importance of energy exchange across the interface to the thermally-driven atmospheric circulation is well established and well recognized in meteorology.

Mr. Johnson first illustrated the order of magnitude of the principal components in the energy exchange, namely the total flux of short-wave radiation,

direct and diffuse, the net flux of terrestrial long-wave radiation at the sea surface, the heat lost by the sea due to evaporation and the flux of sensible heat across the interface, by referring to typical values (Hay[14] and Shellard[15]) of all these components for the ocean weather station "J" (52½° N, 20° W).

He went on to discuss the significance of the distribution of the energy exchange components over two or more oceans according to the computations of Jacobs[16] for the North Atlantic and North Pacific, Budyko[17] for the world's oceans, Albrecht[18] for the Indian and Pacific Oceans and Privett[19] for the oceans of the southern hemisphere. Neglecting secular change of the mean temperature of the oceans and the flux of heat from the interior of the earth, the mean annual net gain of radiation by the oceans as a whole, at the sea surface, must be balanced by the net loss due to evaporation and transfer of sensible heat. Owing to the poleward transport of heat by ocean currents, this balance is not maintained regionally and, for months or seasons, the heat stored by the oceans has to be taken into account. The seasonal and annual studies are generally thought to give the correct order of magnitude of the energy components and which regions of the oceans are generally effective as sources of energy for driving atmospheric circulations. The next meteorological requirements are for studies—none of which have so far been made—of energy exchanges on scales of days or weeks, which studies would be relevant, of course, to medium- and long-range forecasting problems and for providing the data to specify heating functions in numerical forecasting. There is a feedback with the ocean in that such studies of the atmospheric circulations might well call for a parallel treatment of the ocean.

Mr. Johnson explained how in the absence of sufficient direct measurements of solar radiation received at the sea surface various empirical formulae had been deduced in which such factors as cloudiness and sunshine were considered. There were no grounds, however, for assuming that the available formulae would give reasonably accurate estimates of the total solar radiation during periods of days, or weeks, when persistent circulation types prevail. Fortunately several years' total solar radiation measurements are now available for ocean weather stations "I" (59° N, 20° W) and "J" (52½° N, 20° W). Lumb (unpublished) has already made a preliminary examination of hourly fluxes in relation to cloud amount and type during 1960. The results of such studies might be inapplicable to some areas of the ocean due to lack of data, but they might at least be applied over a band of the Atlantic. The albedo of a water surface for direct solar radiation depends upon the altitude of the sun. Budyko[20] gives a table containing values of the average albedo for total solar radiation, expressed as a function of latitude and month; at 50° N the values range from 0.16 in January to 0.06 in June. Jacobs[16] and Privett[19] used somewhat lower values of the albedo due to Schmidt,[21] the annual mean varying from 0.033 at the equator to 0.058 at latitudes 60°, but according to Laevastu[22], who gave actual measurements of albedo obtained on U.S.S. "Rehobot", there was an error in Schmidt's calculations which resulted in the albedo being underestimated. Laevastu's data are in better agreement with Budyko's estimates than with Schmidt's, although they imply that even higher values (0.10 perhaps) might be applicable at 50° N in summer, over the ocean. Most measurements of the reflected radiation have been made over reservoirs and lakes.

Several writers remark that the effect of waves at sea is to increase the amount of radiation reflected, but quantitative allowance has been made only by Hay[14] and Shellard[15]. When the sea freezes the albedo is greatly increased, being 0.5 for sea ice or 0.8 for ice covered with fresh snow according to Sverdrup[23]. In the absence of much needed measurements of long-wave radiation at sea a variety of empirical formulae and constants have been derived for estimating the effective back-radiation, based on measurements made over land. Failing such direct measurements it would be profitable to relate values computed using radiation charts and the weather ship radiosonde ascents, to the surface observations, so that estimates might be made, at least over the main Atlantic shipping lanes, for use in shorter-period energy balance calculations.

Mr. Johnson then considered the exchange of latent and sensible heat by turbulent transfer and described the energy balance method used by Jacobs[16], and later by Budyko[20], Hay[14], Privett[19] and Shellard[15], to compute the average seasonal evaporation from the oceans. Since this is a large and important component of the energy exchange it is unfortunate that rough and ready methods have to be used for its calculation. Measurements of evaporation from pans aboard ships have been made, but are of doubtful use, because their relation with evaporation in the sea is difficult to assess. Laevastu[22] considered that a reduction factor of 0.65 should be applied to the measurements to make them representative of evaporation from the sea; he gave an empirical formula for evaporation based on wind speed, vapour pressure at the sea surface and vapour pressure at a known height above the sea surface and quoted a similar formula due to Penman[24].

Mr. Johnson then described how the energy exchanges across the interface could be computed from the energy balance of the atmosphere and, in particular, mentioned the calculations of Clapp[25] for the normal winter heating in the lower part of the troposphere over the northern hemisphere by the thermodynamic energy equation and by the heat balance method; the difference between the derived distributions by these two calculations is, however, too large for the method to have any practical value as yet and in Clapp's opinion there are large errors in the values computed by thermodynamic energy equations due to neglect of the eddy terms and errors in the computed vertical motions. Dr. Tucker of the Climatological Research Branch is at present working on methods of computing the terms in the thermodynamic energy equations more accurately, in order to derive the patterns of heating for some typical circulation states.

In conclusion Mr. Johnson, following Houghton[26], mentioned that the additional storage of heat or heat deficit, represented by persistent sea temperature anomalies of order one or two degrees Celsius, can be of great importance to the atmospheric circulation. The mean amplitude of the annual variation of sea temperature through the top 100 metres, to the west of the British Isles, is about 4.5° C, which corresponds to a storage and release of 45,000 calories/cm^2, and anomalies of one or two degrees, if they extend to any depth, must represent a sizeable additional heat storage or deficit. The possibility has recently been considered in the Climatological Research Branch of using bathythermograph observations to study the vertical extent of sea temperature anomalies, whether

they are produced in depth by persistent atmospheric circulation types, rather than by the independent action of ocean currents and, particularly, what energy they put into the atmosphere during their life. However, an examination of bathythermograph records indicates unknown changes in calibration, which differ at different depths. There is a need for the accuracy of the bathythermograph observations in depth to be thoroughly tested, and for the true short-period random fluctuations of ocean temperatures to be measured at a fixed location. It is suggested that both purposes would be served by making a series of simultaneous soundings with a bathythermograph and the temperature–depth recorder recently developed at the Fisheries Laboratory, Lowestoft (Booker[27]) for which an accuracy of 0.1° C or better is claimed.

Dr. Robinson pointed out the importance of measuring solar radiation and the radiation balance at the sea surface. During the I.G.Y. solar radiation had been measured at some 400–500 land stations but, at sea, the only observations were at the British ocean weather ships. If detailed observations are really required then every ship carrying meteorological instruments should also have a solarimeter—these are particularly easy to use in tropical seas. Budyko's atlas[17] shows the zonal distribution of the radiation balance, but there is a discontinuity between land and sea over tropical and subtropical seas which has yet to be confirmed by measurement. In other respects the I.G.Y. observations, just mentioned, agreed with Budyko's atlas. *Mr. Charnock* thought that the major difficulty was the unsystematic nature of the observations of radiation. Weather ships apart, they were made sporadically and on random tracks. The number and the quality of marine meteorological observations was insufficient. Serious thought should be given to other observing systems without further delay. *Cdr. Frankcom* said that when the National Institute of Oceanography had offered to make meteorological observations on board *Discovery* he had suggested that solar radiation and net flux of radiation be measured. Such observations had not been considered so far for merchant ships, but he could certainly pick out suitable ships to do this work.

The Chairman stated that Mr. Johnson's paper presented two main problems. The climatic problem (Jacobs/Budyko) was very roughly solved, but the variation of heat balance in space and time from day to day and week to week was a very different problem. *Dr. Robinson* stated that satellites would yield measurements at the top of the atmosphere and ordinary radiation measurements can be made at the surface, but there must be sufficient coverage.

Mr. Sawyer wondered how the input of heat from the sea was to be introduced into the computations of the general circulation and what happens when the climate changes. The formula shown in the detailed presentation (due to Jacobs[16]) depends on sea and air temperatures which are not basic variables. If the sea is advecting more heat what would the heat transfer be? For dynamical calculations in the atmosphere a representative temperature near the surface is required in both air and sea, but it is not certain at what depth it should be; certainly the height in the air must be greater than that of a ship's height over the sea, and the height required for consideration certainly varies in different meteorological situations. *Dr. Ludlam* pointed out that the temperature would vary according to the distribution of cumulus cloud. *Dr. Deacon* said that oceanographers, like meteorologists, are interested in the climatology of the oceans. It even has some bearing on the problem of storm surges described

by Dr. Rossiter earlier in the programme since mean sea level fluctuates by a matter of inches from year to year and over the past 100 years shows an upward trend of about six inches. This begins to be large enough to be important to engineers as well as climatologists. From this and many other points of view it would be useful to know more about secular changes in temperature in oceanic squares where there are sufficient ships' observations and it would be useful if the Meteorological Office could provide facilities for more studies like those made by the late P. R. Brown[28,29]. It would also be useful to have better knowledge and understanding of changes in the large ice-caps which would have a catastrophic effect if they were to melt substantially. There are too few oceanographers and meteorologists working on marine problems, and our ships continually crossing the oceans ought to have more ambitious recording instruments.

General circulation of the oceans

Mr. Crease of the National Institute of Oceanography, in introducing the last subject of the day—the "General circulation of the oceans"—pointed out that although the equations of motion of atmosphere and ocean were basically similar the boundary conditions were certainly not and, in particular, the oceanic longitudinal boundaries prevent the formation of zonal flows except, in a somewhat restricted sense, in the Antarctic. The energy supply of the oceans is almost entirely in the surface layers through the surface winds, by absorption of solar radiation and by exchange of heat with the atmosphere. Resulting from this the physical structure of the ocean is generally typified by a strong thermal gradient (thermocline) in the upper 1000 metres (and usually a salinity gradient also) of high stability while in greater depths the stability is low. He went on to discuss the circulation under four headings, the wind-driven circulation, thermo-haline circulation, Antarctic circumpolar current and transient flux motions of the deep water.

In view of the stability and location of the energy sources the most intense wind-driven circulation is to be expected in the upper layers. In central oceanic regions the flow is diffuse and it is generally assumed that outside the shallow surface frictional layer, about 100 metres or so thick, the motion is essentially geostrophic. Mr. Crease showed how this led to a useful relationship between the meridional transport and the curl of the wind stress. Since all the world's oceans, the Antarctic apart, are bounded effectively at their northern boundaries the total meridional transport may be found from the wind stress curl—neglecting any transport from ocean to ocean through the Arctic. However, details of the wind stress over vast areas are not well known for at least three reasons: lack of coverage of the area, the difficulty of wind velocity measurements and the deduction of $U*$ (friction velocity) from the observations.

Mr. Crease mentioned the difficulty that this equation relating meridional transport and the curl of the wind stress, does not allow for there being no mean net flow out of a closed basin. One solution of this problem by Stommel[30] Munk[31] and others deals with lateral friction but neglects relative vorticity; this leads to the interesting conclusion that a frictional boundary current (by itself) is possible only on the western boundary—the presence of strong western currents is an established fact, e.g. Gulf Stream, Kuroshio. A second solution

(Charney[32], Morgan[33]) which considers the vorticity but not friction is consistent with the observed motion in low latitudes. Possibly because the frictional theory was the first to appear and gain acceptance, the possibility of eastern boundary currents has been somewhat neglected. Recently Carrier and Robinson[34] have discussed a full inertial theory which clarifies the situation. Their results lead to the interesting conclusion that there must be a strong zonal jet at the latitude of maximum zonal wind stress gradient. As a dissipating mechanism for the energy input they introduce narrow frictional boundary layers embedded in the inertial layers and further find that the region where the western boundary current joins the zonal jet must be a region of particularly large turbulence. It is just about here that the Gulf Stream appears to become very unstable.

Mr. Crease went on to show what could be deduced regarding the flow of currents from a knowledge of the temperature and salinity variation with latitude and depth and referred in particular to a diagram, obtained from the results of Wüst[35], which gave a simplified meridional section of the eastern basin of the Atlantic. Such temperature and salinity data are sufficient to define the topography of given pressure surfaces provided that the topography of the sea surface can be deduced by independent means. In principle the simple relationship with the wind stress curl, which he had mentioned earlier, is sufficient, together with the density distribution, to determine the sea surface profile but lack of knowledge of the wind stress curl and the problem of deciding what would be appropriate average processes make the method difficult to apply. Other indirect methods are used, which, with the continuity equation applied across a zonal section of the ocean, establish a plausible mid-depth level of no meridional flow (Wüst[35]) which may be used as a reference depth for the estimation of the geostrophic flow in the interior. Mr. Crease outlined the thermo-haline circulation obtained by Stommel[36] and Stommel and Aarons[37] who extended the assumption that the flow is geostrophic, except at western boundaries, to a two-layer model of the ocean, which he presented in diagrammatic form. It is established that the only two major localized sources of deep water are the Antarctic bottom water and the North Atlantic deep water. There are no sources in the Pacific or Indian Oceans. Predictions, from this model, of boundary currents in the major oceans have so far been tested against observations only in the North Atlantic (Swallow and Worthington[38]); a southerly flowing current was found under the Gulf Stream but later work suggests that a long series of observations will be required to establish the existence of a mean flow. The implications of inertial theories also remain to be considered. The boundary currents suggested by the model provide a possible explanation of the observed difference in surface flows in the North and South Atlantic; in the first case the wind-driven circulation is augmented by the upper layer of the thermo-haline flow and in the second it is diminished. The vertically integrated transport remains the same in both hemispheres.

The last two aspects of the circulation dealt with by Mr. Crease were those of the Antarctic, which provided the nearest approach to an unrestricted zonal ocean, and the recently discovered transient flux motions of the deep ocean. While observations are scarce in the Antarctic an outstanding feature is the Antarctic convergence (likened to the polar front—Eady[39]) at which surface waters flowing with a northerly component (due to the zonal wind stress of the

westerlies) sink beneath water of subAntarctic origin (Deacon[40]). A number of crossings of the convergence suggest that its position is stable to within a degree or two, so that perhaps the deeper waters exert the controlling influence upon it rather than the wind system (at the latitude of the convergence the deep water is rising over the north-flowing bottom water). The westerlies result in a general westward flow on both sides of the convergence as far south as 65°S where the prevailing easterlies give a reverse flow along the continental boundary. The dynamics are not well understood and Mr. Crease mentioned the different view of Stommel[36] and Wyrtki[41].

Finally Mr. Crease mentioned the important recent discovery, by measurements in the western Atlantic, of transient currents in the deep water with velocities of the order of 10 cm/sec; they appeared to have time scales of at least a week or two and space scales of the order of thirty miles (Crease[42]) and are therefore at least one order of magnitude larger than the estimated mean flow, so that they certainly must be taken into account in considering the deep circulation in relation to the mean flow.

In reply to a query from *Dr. Sutcliffe* regarding the shape of the transient currents, *Mr. Crease* said the measurements were too few but with the present measurements there appeared to be no noticeable correlation in the east–west and north–south components of the transients; this matter certainly required further looking into as such currents might contribute significantly to the flux in deep water. Meteorologists could join in this study of the circulation and transfer at the sea surface.

Dr. Hunt pointed out that the thermal and wind-driven steady circulations are not the only significant mechanisms for energizing such currents. By virtue of the non-linear terms of the equations of motion, the tidal oscillations generate second-order currents which are functions of position. This is a three-dimensional extension on a tidal scale with Coriolis terms included, of the work on wave drift currents by Stokes[43] and more recently by Longuet-Higgins[44]. These tide-induced currents can be calculated from co-tidal and co-range charts for each tidal constituent and in many areas contribute significantly to the observed circulation. There are also small vertical components vanishing naturally at the surface and at the bottom, but at mid-depths they are of the order of magnitude of the flow through the thermocline quoted by Mr. Crease, but not always of the same sign. They result in circulations in vertical sections similar to the results of Rayleigh[45] and Longuet-Higgins[44] for small-amplitude two-dimensional standing waves. As an example, a calculation of the tide-induced bottom currents in the North Sea produces a current chart which is very similar to the observed surface current chart but with the velocities reduced by 30–50 per cent. *Mr. Crease* stated that transient currents give similar motion but he was surprised to learn that they gave so large an effect.

The Chairman asked what was the order of the tidal currents in the open sea and *Mr. Crease* stated that this was 10 cm/sec. *The Chairman* wondered whether there was any other technique for studying transient currents besides that used by Swallow[38]; experiments could be made at the ocean weather ships. *Mr. Crease* said that at the National Institute of Oceanography they had considered the possibility of studying a 100-kilometre square of ocean with uncomplicated topography; there would be moored buoys with bottom instruments and also an above-surface buoy recording the heat balance. *Dr. Tucker* said that in some

ways the general circulation of the atmosphere appears simpler than that of the oceans, but perhaps this is merely because observations are easier to make. A profitable line of inquiry in atmospheric studies has been the study of the amount of potential energy to kinetic energy conversion necessary to maintain the flow against the dissipating effect of surface friction and the direction of transfer between these two energy forms in the scale of eddy size. However, all motions of meteorological interest in the atmosphere are thermal in origin, whereas in the ocean frictional, thermal and saline effects are all important. Is it then possible, or better profitable, to formulate similar questions in oceanography? This approach could be important as regards the existence and effects of the transient systems mentioned by Mr. Crease. In the general circulation of the atmosphere we have been able to study energy conversion and energy fluxes with reference to three broad scales of motion: meridional motion, "standing" eddies and "local" eddies. The latter presumably correspond to possible transient oceanic systems, while "standing" eddies are analogous to major ocean currents. Oceanic observations obviously do not exist in sufficient accuracy, quantity or distribution in space for similar detailed examinations; but can the principles of this type of study be applied qualitatively to see if transient systems are necessary from dynamical or energy considerations? *Mr. Crease* said they would like to study the dynamics of such a limited area as he had mentioned but he wondered if the transient currents were on this scale. *Dr. Sutcliffe* wondered why the name "transient" had been used—are they there all the time? *Mr. Crease* said that when the National Institute of Oceanography were working off Bermuda last year there was one trajectory for a period of a week, say to the north-east and a week later in the same area the current might well be to the south-west (the investigation was made by having floats at certain depths, some currents were at 2000-metre depths and others at 4000-metre depths). His impression was that the Lagrangian scale was larger than the Eulerian.

Mr. Jones and *Dr. Ludlam* pointed out these variations also occurred in atmospheric motions, e.g. as recorded by smoke puffs. *Dr. Sutcliffe* asked whether these transient currents were geostrophic. *Mr. Crease* said they were usually, but not always. One example was when the 2000-metre speed was half a knot and the 4000-metre speed was one knot, the pressure gradient showing nothing like this change. *Dr. Ludlam* said we cannot continue to deplore the lack of data, there must eventually be complete synoptic networks. *Mr. Tunnell* wondered why more use was not made of the nine ocean weather ships. In discussions with Dr. Swallow of the National Institute of Oceanography it appeared feasible to survey the bottom of the oceans under the ocean weather ships and then to do soundings with floats up and down. *Mr. Crease* said they would have a similar problem with *Discovery* in the Indian Ocean Expedition; the proposal was to moor buoys and use notable points on the sea bottom as fixed markers.

Mr. Charnock drew attention to the difficulties which arise in present theories of the main thermocline if baroclinic disturbances are transferring heat upwards.

Dr. Angell remarked that oceanographers use trajectory methods first and consider synoptic methods later, whereas meteorologists have gone the reverse way. *Mr. Crease* said he would welcome the meterologists' long experience in this general problem.

Mr. Crease wondered whether radiation thermometers would be of use to the oceanographers in their general study. *Dr. Murgatroyd* said that the Meteorological Research Flight had made no such measurements but measurements from the air had been made under the guidance of the Woods Hole Institute. *Mr. Hay* gave a few details of the American airborne radiation thermometer. Although this instrument is equipped with an improved bolometer it is still in the development stage and calibrations before and after flight trials have shown that uncertainties in readings are of frequent occurrence.

The Chairman summarized the day's discussion by stating how useful it had been for the oceanographers and meteorologists to come together to discuss the various problems which had been outlined in the four topics. The present formal meeting should lead to many informal future meetings between those concerned with similar problems, e.g. into the use of electronic computers for solving the equations of motion.

Dr. Sutcliffe, on behalf of the Meteorological Office, thanked all concerned for the work they had done in preparing and giving the papers and contributing to the discussion. He would be very pleased to arrange for any co-operation which the Meteorological Office could give in furthering research into this most important problem of the "Interaction between the atmosphere and the oceans".

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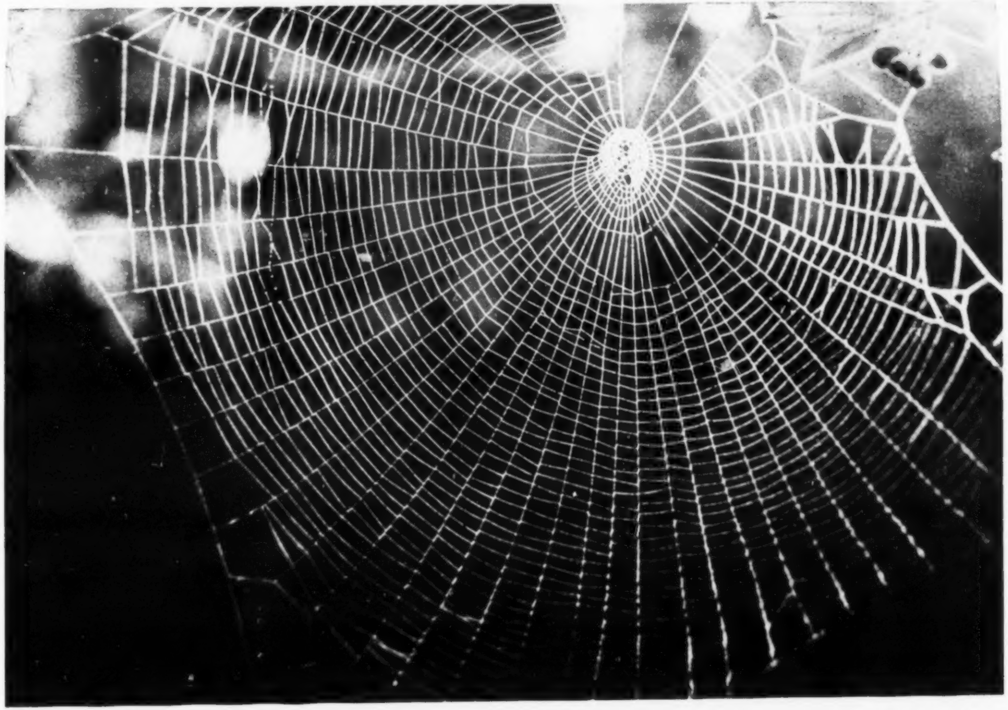


Photograph by R.K. Pilsbury

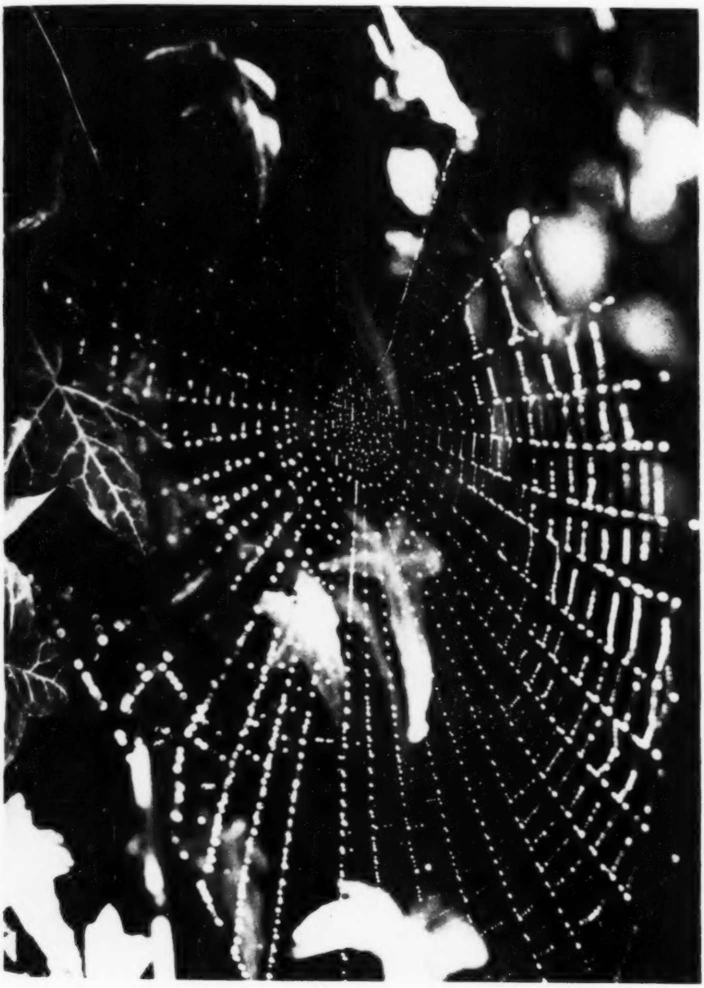
PLATE I—DROPLET-LADEN COBWEB AT 0705 GMT, 31 AUGUST 1961

(see p. 239)

Photograph by R. K. Pilsbury

PLATE II—DROPLET-LADEN COBWEB AT 0710 GMT

(see p. 239)



Photograph by R. K. Pilsbury

PLATE III—DROPLET-LADEN COBWEB AT 0730 GMT

(see p. 239)

To face p. 225

Photograph by R.K. Pilsbury.

PLATE IV—DROPLET-LADEN COBWEB AT 0735 GMT

(see p. 239)

PLATE IV—DROPLET-LADEN COBWEB AT 0735 GMT
(see p. 239)

WORLD METEOROLOGICAL ORGANIZATION

Third Session of the Commission for Synoptic Meteorology

By V. R. COLES

A little more than four years after its second session in New Delhi, the World Meteorological Organization Commission for Synoptic Meteorology (CSM) met for the third time on 26 March 1962. The conference was held in Washington and lasted for four weeks, with a total of over 70 representatives from 40 countries. The United Kingdom delegation consisted of Messrs. Coles, Starr and Harding from the Meteorological Office and Instr. Capt. Burnett from the Naval Weather Service.

All meetings took place in the International Conference Suite of the State Department Building which has recently been completed and is designed and fully equipped for such gatherings. The Assistant Secretary of State for International Affairs, Mr. Harlan Cleveland, welcomed the delegates on behalf of the United States and the conference then settled down to its work under the presidency of Mr. P. Kutschenreuter of the United States with Dr. S. N. Sen of India, as his deputy.

As at previous sessions, three committees were established. One was concerned with codes, another with telecommunications and the third with matters falling under neither of these headings. A number of items were inevitably considered by more than one committee.

There were long debates concerning proposed changes in both surface and upper air codes but it was finally decided that, in view of the many difficulties and problems involved, there should be no major changes in codes for the time being. However, the Commission decided that the Working Group on codes should be re-established to make recommendations for changes in both surface and upper air codes in time for the proposed new codes to be tested by all Members of the World Meteorological Organization before the next session of the Commission. A few minor changes in codes were, however, agreed. It was decided that sections 1, 10 and 11 of TEMP messages should be exchanged internationally and that indicator letters within the TEMP messages are essential for purposes of forecasting by electronic computers. In the SYNOP code it was agreed that the height of the base of low cloud, reported in the fifth group, should *always* refer to the lowest cloud in the sky, and agreement was reached on the definition of a squall for reporting purposes under figure 18 of the present weather code. However, the Commission was unable to come to a definite decision concerning the use of the metric unit of metres per second in place of knots for reporting wind speed because it was felt that the conversion from metres per second to knots for aeronautical purposes, although an easy one to make, involved a risk of error. The Executive Committee was therefore requested to take the appropriate steps to resolve the conflict.

The Commission also considered the reports of the other Working Groups established at the second session of the Commission. The report of the Working Group on the Guide to Synoptic Meteorological Practices was considered to correspond to the needs expressed at the second session of the Commission subject to certain amendments in order to incorporate decisions adopted at the 1962 session of the Commission. However, the methods to be used for the reduction of pressure to mean sea level, which were recommended by the Working Group

on pressure reduction methods, were considered by members to be rather complicated and it was decided that it is premature, at present, to recommend any method of pressure reduction for universal use. The recommendations contained in the report of the Working Group which was set up to examine and define quantitatively the terms used to describe the intensity of meteorological phenomena were accepted in the main though it was realized that other Technical Commissions of the World Meteorological Organization may be affected by such changes and may wish to comment before they are introduced. The recommendations also affect the present weather code so that, even if they are accepted by other Technical Commissions, they cannot be introduced before the next session of the Commission for Synoptic Meteorology. One of the most interesting reports to be considered came from the Working Group set up to consider the synoptic use of meteorological data from artificial satellites. The Commission agreed with the recommendation of this Working Group that the desirable minimum frequency of observations over any given area for general synoptic purposes should be every six hours and within three hours of the actual observations. Furthermore, the Commission recommended that the Secretary-General of the World Meteorological Organization be requested to endeavour to organize seminars on the synoptic use of meteorological satellite data at various locations throughout the world. The report of the Working Group on the forecasting of hail, turbulence in clear air and in cloud, icing and dense cirrostratus clouds was published before the third session of the Commission in the form of four excellent Technical Notes—numbers 37 to 40 inclusive.

Apart from the re-establishment of the Working Group on code matters several other Working Groups were re-established, including those on meteorological satellites and networks. In addition three new CSM Working Groups were established—one on qualifications and training of meteorological personnel, one on long-range forecasting, and a joint Working Group with the Commission of Aerology on numerical prediction.

The telecommunications committee spent much of its time considering the distribution of data in the northern and southern hemispheres. Five stations are involved in the northern hemisphere and three in the southern hemisphere and arrangements to provide linkages between the two hemispheres with the aim of affording synoptic world coverage were also discussed. The large volume of telegraphic traffic engendered by the interchange of meteorological data and the still larger volumes envisaged in the future, including TIROS and NIMBUS satellite data, raises the question of the use of higher telegraphic speeds. No firm position could be taken on this matter at the third session of the Commission but there exists, fairly widely, the feeling that faster speeds should be operated as early as economically practicable. A good deal of time between sessions and in the Working Group on meteorological telecommunications has been devoted to the standardization of the format of meteorological messages. This is particularly important to the electronic processing of coded data, either with an editing and re-routing function or for analytical work such as numerical forecasting. The third session of the Commission approved, with small modifications, the proposals of the Working Group. In the fullness of time an approved version of the new format will appear in all teleprinter page copy within the Office. Simultaneously, outstations and, in particular, teleprinter collecting centres

will be requested to adopt with scrupulous care the same format in their transmissions to Bracknell, for the value of such a format resides almost entirely in its universal application.

Towards the end of the session Dr. S. N. Sen of India, was elected as the new President of the Commission with Dr. Logvinov, of the U.S.S.R., as Vice-President.

There was much appreciation for the hospitality of our American hosts who arranged sightseeing tours of Washington and Mount Vernon, a visit to the White House and a picnic at the Gambrill State Park in Maryland as well as tours of the Weather Bureau National Meteorological Centre and Meteorological Satellite Activity at Suitland, Maryland and the Goddard Space Flight Centre.

551.524.36

FIVE-DAY MEANS AND EXTREMES OF MAXIMUM AND MINIMUM TEMPERATURE AT LONDON AND GLASGOW

By M. H. FREEMAN, O.B.E., M.Sc.

A standard programme has been drawn up for the processing of surface observations punched on cards. One of the items is concerned with five-day means and extremes of maximum and minimum temperatures. For each five-day period, 1–5 January, 6–10 January, etc. (omitting 29 February), the daily maximum temperatures in the ten years 1949–58 were listed in order of magnitude, and the mean of the 50 values was calculated. From the list were read off the highest and lowest values and also the tenpercentile (decile) values. (Ten per cent of the temperatures are greater than the upper tenpercentile reading and ten per cent are lower than the lower tenpercentile reading). Similar data were extracted for the daily minimum temperatures.

In Figures 1 and 2 are shown graphs of the mean, the extremes and the tenpercentile (decile) readings for London (Heathrow) and Renfrew Airports. As would be expected the extreme values show considerable irregularity; the period is much too short for any significance to be attached to the individual peaks and troughs. The tenpercentile lines are less irregular and give a useful guide to a forecaster of the range of values within which he can fairly confidently confine his forecast of maximum or minimum temperature. He should only make a forecast outside these ranges when he has good grounds for so doing.

The diagrams are published to illustrate the kind of results to be expected from ten-year means. Nevertheless the curves also show several features of general interest. In summer the range of values taken by the maximum temperature is greater than that for the minimum temperature, and in the winter the reverse is true. The rate of drop in maximum temperature in autumn is greater than the rate of rise in spring; and the rather small range of maximum temperature in autumn is interesting.

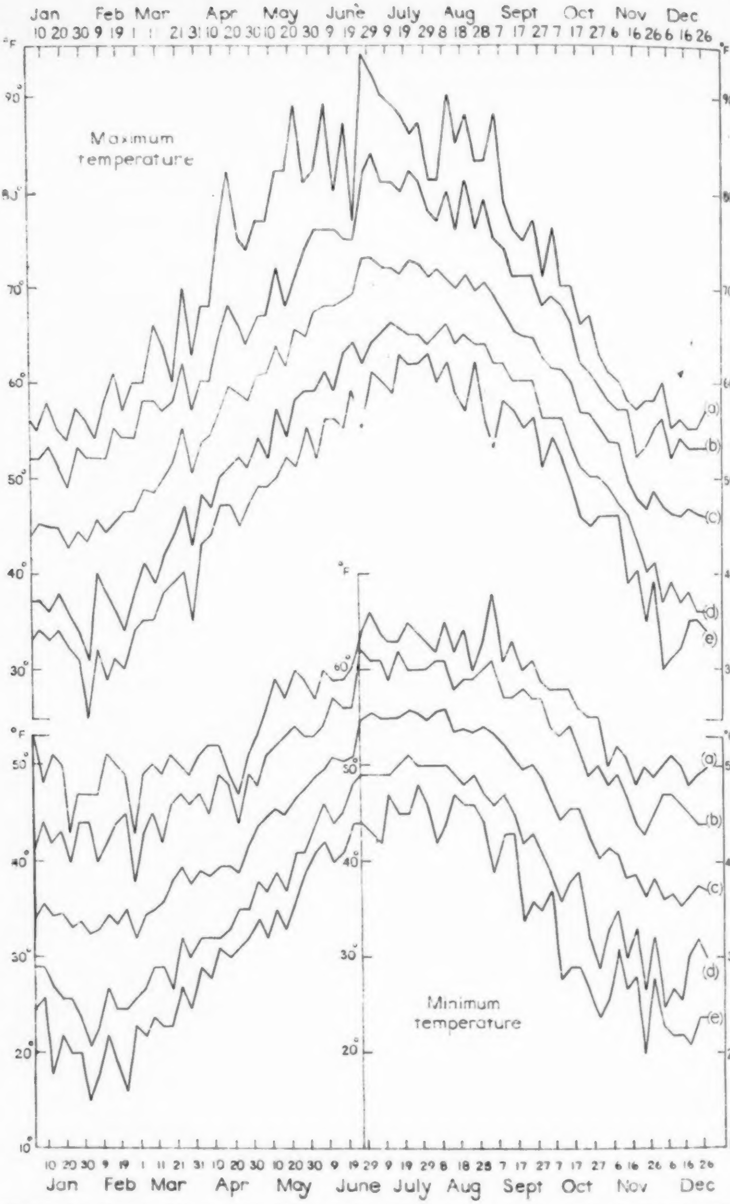


FIGURE I—FIVE-DAY MEANS, EXTREMES AND UPPER AND LOWER TENPERCENTILES OF MAXIMUM AND MINIMUM TEMPERATURE FOR LONDON (HEATHROW) AIRPORT, 1949–58

(*a*) upper extreme, (*b*) upper tenpercentile, (*c*) mean, (*d*) lower tenpercentile and (*e*) lower extreme.

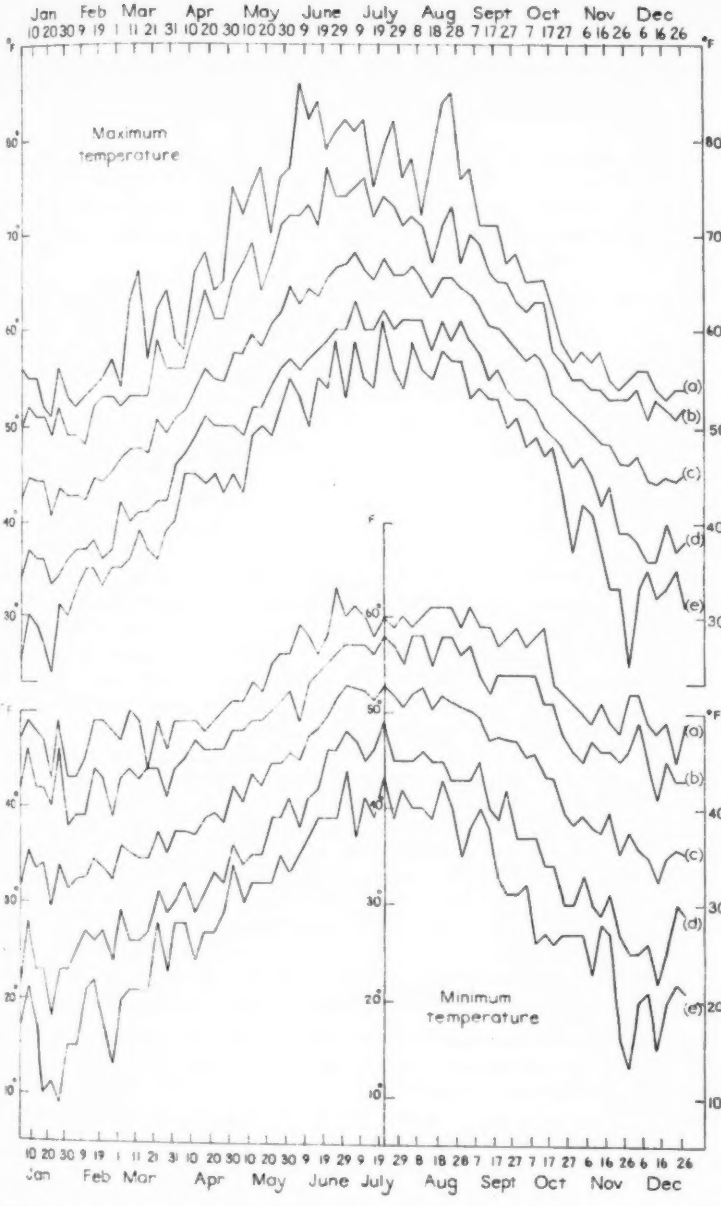


FIGURE 2—FIVE-DAY MEANS, EXTREMES AND UPPER AND LOWER TENPERCENTILES OF MAXIMUM AND MINIMUM TEMPERATURE FOR RENFREW AIRPORT (GLASGOW), 1949–58

(*a*) upper extreme, (*b*) upper tenpercentile, (*c*) mean, (*d*) lower tenpercentile and (*e*) lower extreme.

551.594.221

A LIGHTNING DISCHARGE TO A BALLOON AT CARDINGTON

By J. HODKINSON

An incident occurred at Cardington during the "Balthum" sounding at 0600 GMT on 14 December 1960, when a lightning discharge took place through the flying cable of the captive balloon and minor damage occurred to the recording equipment during conditions in which no other thundery activity was reported. The probable causes of the incident are discussed below.

During the 0600 GMT "Balthum" ascent on 14 December 1960, when the balloon had reached a height of about 2750 feet above ground, a blue flash was seen to jump from the steel flying cable to earth; at the same time minor damage was sustained in the recording equipment when a switch and two indicator lamps burnt out. No other damage to the balloon or instrumental equipment took place, and no personal injury occurred. At the time of the incident the weather was overcast with a moderate sleet shower in progress; prior to this time the night had been foggy with some very slight rain and drizzle. There were no other reports of lightning and no atmospherics were reported. A lightning flash counter installed at Cardington responded only to this single flash. At the time a high-pressure system over Scandinavia was linked through a ridge over the British Isles to another anticyclone near the Azores and an easterly airstream was maintained over Cardington. This airstream was unstable in the lower layers and was carrying some wintry showers from the North Sea into the Midlands. The Hemsby upper air soundings made at 2330 GMT, 13 December and 1130 GMT, 14 December, and reproduced in Figure 1, indicated that cumulus and stratocumulus cloud probably existed in layers up to about 6000 feet with occasional cumulus tops reaching to not more than 10,000 to 12,000 feet in association with showers. There was fairly extensive fog and low stratus below the main cloud layers. The lightning risk was forecast as "poor chance".

It is evident that an electric discharge occurred down the steel flying cable which the earthing system was unable to handle. There is inevitably a small resistance to earth so that temporarily the cable near the ground attained a high potential of sufficient magnitude for sparking to take place. The damage in the recording equipment was due either to a similar discharge taking place in the auxiliary recording cable or from induced currents generated by the large fluctuating currents in the neighbouring steel flying cable.

It is generally accepted that the main risk of lightning discharge to captive balloons occurs in association with the large space charges and the associated high potential gradients present in or near large cumulus or cumulonimbus clouds. Simpson[1] has examined the records of atmospheric electricity made at Kew during disturbed weather and has shown that very high fluctuating potential gradients were found in association with snow showers. It seems likely that during snow showers high potential gradients may occur in association with the space charges in cumulus of only moderate vertical extent and that these potential gradients may be approaching the same order of magnitude as those found near active thunderstorms where the cloud is usually of a much greater vertical extent.

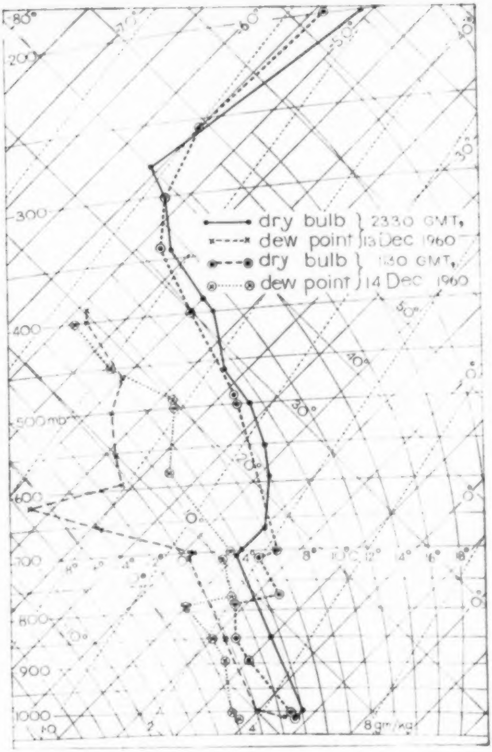


FIGURE 1—HEMSBY UPPER AIR ASCENTS FOR 2330 GMT, 13 DECEMBER AND 1130 GMT, 14 DECEMBER 1960

Davis and Standring[2] investigated the discharge currents found in the flying cables of captive balloons. They point out that the earthed flying cable, during this incident 2750 feet long, will short out an appreciable part of the gap between the cloud charges and the ground. Under these circumstances discharges to the balloon may occur from cloud charges which are too small or too widely distributed to produce lightning strokes to the open ground. The presence of the balloon may thus initiate lightning strokes where they would not otherwise take place and it is probable that this is the explanation of the incident at Cardington. It is probable that when precipitation is in the form of snow or sleet the risk of a similar incident may be greater than if it were in the form of rain showers. Two cases are on record where balloons were destroyed by lightning during snow showers but in each case a warning of high lightning-risk was in force.

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1. SIMPSON, SIR G.; Atmospheric electricity during disturbed weather. *Geophys. Mem., London*, **10**, No. 84, 1949.

2. DAVIS, R. and STANDRING, W. G.; Discharge currents associated with kite balloons. *Proc. Roy. Soc., London*, **191**, Series A, 1947, p. 304.

551.575.1 : 551.591.36 : 551.43

SOME ASPECTS OF THE FORMATION OF FOG OVER HIGHER GROUND

By W. R. SPARKS

Introduction.—The problem of forecasting radiation fog at stations above the general level of the surrounding countryside is a complicated one. Forecasters at aerodromes on higher ground were requested to study local variations in fog formation, and several hypotheses were put forward to explain the processes at work. A questionnaire was circulated to assist outstation forecasters to record observations, and to ensure that the data collected would be in a form amenable to statistical analysis. The questionnaire was designed to yield information on the formation of fog in valleys around aerodromes and on the deepening of the fog to affect the higher ground. A total of 81 completed, or partially answered, questionnaires were returned by eight stations. Sufficient data were obtained to test only one of the hypotheses statistically, but individual cases supporting other hypotheses were found.

Formation of fog in valleys.—It is supposed that on radiation nights saturation usually occurs first within a few feet of the ground and fog then forms there rapidly. However, on some occasions, probably mainly when an inversion is already present and the air is polluted, cooling is spread more uniformly through a layer several hundred feet deep and fog then forms more gradually through a deep layer. The observations given in Table I support this hypothesis.

TABLE I

	No. of cases of gradual formation of fog	No. of cases of rapid formation of fog
Polluted air	13	6
Unpolluted air	2	16

The chi-squared test shows that the probability of this distribution occurring by chance, if pollution has no effect on fog formation, is less than 0.001.

Night cooling and fog in irregular terrain.—In the valleys and lower-lying areas within an area of moderate or strong relief, radiation fog may form in the usual way, but the low wind speed in the inversion layer and the increased density of the air may be such that the air is unable to surmount the neighbouring hillsides and is trapped in the valleys. When such stagnant pools of cold air have been formed in valleys, the air above them will continue to move under the influence of the large-scale pressure field (i.e., geostrophic wind) and the flow may indeed be stronger than during the previous day because of the reduced loss of momentum by turbulence. If the depth of the fog is less than the height of the hills this steady airflow will envelop the hilltops and the upper part of the slopes, and there will be little cooling in it because of its relatively brief contacts with the ground and it will be fog-free.

This hypothesis is supported by observations from Waddington and Swinderby on 1 November 1958. Waddington (231 feet above M.S.L.) and Scampton (215 feet above M.S.L.) are near the crest of the Lincoln Edge. The land slopes gently away to the east, but to the west there is a steep slope, with a drop of 150–200 feet on to the Witham–Trent plain. Swinderby (69 feet abov M.S.L.) lies on this plain, about seven miles west-south-west of Waddington. At 0001 GMT on 1 November 1958 there was a ridge of high pressure over the

British Isles giving a north-westerly gradient wind over Lincolnshire. The surface wind at Swinderby became calm soon after midnight, and fog formed between 0100 and 0200 GMT at a temperature of 32°F; the minimum temperature in the screen was 29.6°F. At Waddington the surface wind was between west and south-west, 5–7 knots, throughout the night; there was no fog, and the minimum temperature in the screen was 33°F. There was never more than one okta of cloud at either station between midnight and dawn.

The steepness of the windward slope is a very important factor affecting airflow over a ridge or hill at night. A steep-sided valley is much more efficient in trapping cold air than a valley with gently-sloping sides, and it is supposed that the slow ascent of air up a gentle slope aids the formation of radiation fog.

An investigation was made locally at Scampton using data for the two years September 1953 to August 1955 inclusive. (Owing to the closing of Scampton, observations from Waddington were used for the last three months). In a second investigation use was made of data from Waddington and Swinderby for the three winters from November 1958 to February 1961 inclusive. Data from both investigations have been combined to give the result described in the following paragraphs.

There were 45 occasions when the surface wind across the Lincoln Edge had a westerly component and radiation fog formed at both high- and low-level stations. The average time of formation was 1.0 hours *earlier* at Swinderby than at the station on the Edge. There were 20 occasions when the surface wind across the Lincoln Edge had an easterly component and fog formed at both high- and low-level stations. The average time of formation was 0.8 hours *later* at Swinderby than at the high-level station.

These results are in accordance with the supposition that a steep slope to windward delays the formation of radiation fog at a high-level station, while a light wind up a gentle slope accelerates the formation of radiation fog at the top of the slope.

Characteristics of the airflow and effect on fog distribution.—The conditions of inversion and little turbulence on radiation nights are particularly favourable for the development of steady streamline flow over hills as described by Corby[1]. Some of the characteristics of such flow in relation to the local

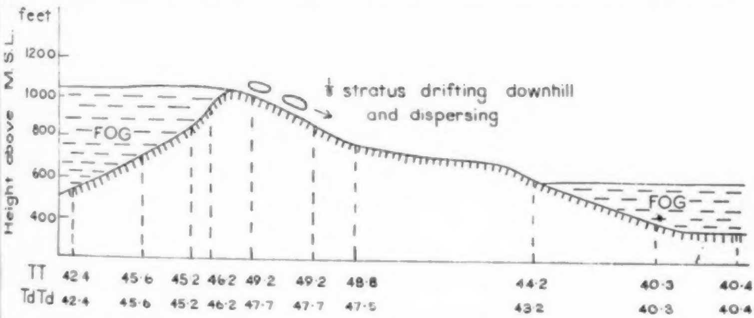

FIGURE 1—CROSS-SECTION OF VIEW EDGE ESCARPMENT SHOWING THE DISTRIBUTION OF FOG AND TEMPERATURE ON 25 SEPTEMBER 1960

TT = dry-bulb temperature in °F $\qquad$ T_dT_d = dew-point in °F

topography may have important effects on the fog distribution. Notable is the tendency for the flow to be strengthened on the lee slope of the hill, the wind blowing down the lee slope and following the ground contour. It may be supposed that lee slopes of hills will often be fog-free while the foggy stagnant inversion layer is increased in depth on the windward slope.

Observations supporting this hypothesis were made by Mr. J. E. Atkins on 25 September 1960 on the slopes of the View Edge escarpment in the vicinity of Craven Arms, Shropshire. The synoptic chart for 0600 GMT on 25 September 1960 showed an anticyclone centred over Northern Ireland, with a ridge extending south-east over Wales and southern England. The pressure distribution did not appear to favour any definite wind over Craven Arms. Figure 1 shows a cross-section of the fog and temperature distribution on the two slopes of the escarpment.

The main features of the observations made between 0618 and 0707 GMT were as follows:

(*a*) Conditions were generally calm except on the upper part of the dip slope where there was a wind of force 2 from west-north-west (i.e., down the slope).

(*b*) There was a difference of about 400 feet between the fog top over the scarp edge and the dip slope. Over the scarp edge the fog top was more or less level with the top of the escarpment, but patches were drifting over the crest and moving down the dip slope as small amounts of thin stratus which rapidly dispersed on descent.

(*c*) There was a difference of about 4°F between temperatures at corresponding levels on opposite sides of the escarpment, temperatures on the dip slope being the higher.

(*d*) There was a temperature inversion, on both slopes, of the magnitude that might be expected in the free air after a night of radiation fog.

A second run was made between 0840 and 0917 GMT as the fog was dispersing, mainly from the top downwards. On the eastern side of the escarpment (facing the sun) all that remained was mist at the lowest levels. On the western side, which was shaded by the escarpment, the fog top had lowered by some 300 feet. There was no longer a stronger wind near the crest than elsewhere—a very light wind (force 1) from the north-west affected all parts. The temperature contrast between the scarp edge and the dip slope not only persisted but extended down to the bottom of the slopes. The contrast was well illustrated by the fact that to the west of the escarpment lay an unbroken area of fog, whereas above the dip slope (facing the sun) convection had commenced and small cumulus clouds were forming. Above the mist and fog, visibility was very good.

Effect of the changing wavelength of lee waves on fog at a high-level aerodrome.—Under conditions of streamline flow the formation of lee waves is possible and this may cause variations in the depth of the foggy inversion layer to the lee of a ridge of hills.

The observations in Table II, made at Bovingdon on 7 December 1960, support this hypothesis.

It will be noted that each fall in visibility is accompanied by a drop in temperature and wind speed, while an increase is associated with an increase in wind speed and a rise (or reduction in the rate of fall) of temperature.

TABLE II

Time GMT	Wind deg/kt	Visibility yd.	Dry-bulb °F	Dew-point °F	Cloud amount	Cloud type	Cloud height (ft)	Remarks
1356	360/08	2200	39·1	38·5	7/8	St.	900	
1455	360/05	1800	37·8	36·3	1/8	Sc.	2,000	
1541	350/02	900			1/8	Ac.	12,000	
1553	340/02	900	33·4	33·2	1/8	Ac.	12,000	
1630	330/05	1100			1/8	Ci.	20,000	
1653	320/05	1200	32·4	32·2	1/8	Ci.	20,000	
1755	330/06	1300	33·3	32·8	1/8	Ci.	20,000	
1855	340/02	400	29·0	29·0	Nil			Fog 1850–1938
1938	330/03	1200						
1953	350/04	1100	31·0	30·7	Nil			
2027	350/02	400			Sky obscured			

It is supposed that lee waves, due to the topography upwind, were causing undulations of the fog top in the Bovingdon area, and that a gradual shortening of the wavelength caused the aerodrome to be affected alternately by crests and troughs in the wave pattern. Thus when Bovingdon was in a wave trough it was in relatively warm, free-flowing air, and when under a wave crest it was affected by the slower-moving foggy air. Examination of the Cardington BALTHUM (special reports of wind and temperature) for 1200 GMT on 7 December 1960 showed that lee waves, of short wavelength, were possible during the afternoon.

Conclusion.—Insufficient information was available to test the hypotheses thoroughly, but the investigation gave some support to them. The problem is a complicated one and many more data will be required. There is therefore a continued need for local investigations which pay due attention to station peculiarities; at this stage the most useful additions to our understanding of the problem will come by detailed field studies of individual occurrences.

REFERENCE

1. CORBY, G. A.; Air flow over mountains. *Met. Rep., London*, **3**, No. 18, 1956.

551.586 : 63

WEATHER AND FOOD

By R. G. VERYARD

"They take the flow o' the Nile
By certain scales i' the pyramid; they know
By the height, the lowness, or the mean, if dearth
Or poison follow. The higher Nilus swells
The more it promises; as it ebbs, the seedsman
Upon the slime and ooze scatters his grain
And shortly comes to harvest"

Antony and Cleopatra, II, VII, 20.

From time immemorial whether as hunter, farmer or fisherman man has had to cope with the elements in his search for food. Through the process of trial and error involving many painful experiences including mass starvation and forced migration, by domesticating and cultivating wild fauna and flora, by accumulating knowledge of the nature of seed, soil and weather, and so on, man has been able to maintain an ever-growing population. But there is still not enough food to go round. Although there are occasional gluts in some countries (incredibly, the surplus crop is sometimes destroyed!) there are millions who are

not adequately fed—yet within a few decades the present world population may have doubled itself! It is understandable therefore that increasing attention is being paid to the problem of feeding not only the undernourished peoples of today but the many more millions who will need feeding in the years to come.

Several of the United Nations special agencies are concerned with this problem. As long ago as 1951, UNESCO established an Arid Zone Programme aimed at the collection and dissemination of the results of research on arid zone problems and the giving of direct assistance to projects forming part of a co-ordinated research programme. Much money has been spent and a perusal of the "Arid Zone" newsletters and Reviews of Research published by UNESCO may give some idea of the progress achieved to date. As far as food production is concerned the progress must be regarded as rather disappointing. However, the United Nations body chiefly concerned has been FAO, the Food and Agriculture Organization. In July 1960, it launched a Freedom from Hunger Campaign, and at the inaugural ceremony in Rome, Mr. B. R. Sen, the Secretary- General of FAO, emphasized that "One man's hunger and want is every man's hunger and want; one man's freedom from hunger and want is neither a true nor secure freedom until all men are free from hunger and want". The Campaign is to continue until 1965 and is aimed at the progressive and lasting removal of hunger and malnutrition from the human scene and not merely at the provision of temporary relief. The United Nations and the other specialized agencies, including the World Meteorological Organization (WMO), were invited to contribute to the Campaign.

WMO had previously agreed to arrange joint FAO/WMO projects related to the application of meteorology to agriculture and had already undertaken to participate in a FAO/UNESCO/WMO project on agroclimatology in the arid and semi-arid zones of the Mediterranean region. In regard to this project a rather optimistic article in the January 1961 issue of the *WMO Bulletin* expressed the hope that the findings would result in "practical recommendations for extending or introducing into the region crop species with plant requirements which are met by the climatic conditions of the countries to be studied" and envisaged that "along with improved field cultivation and irrigation systems, this could certainly lead to an important increase of the native food production". It remains to be seen how successful this project will be.

In response to the FAO invitation to participate in the Freedom from Hunger Campaign it was decided by the WMO Executive Committee that the main contribution of WMO should consist in the preparation and publication of a booklet entitled *Weather and food**. The intention was that this publication should be in a style suitable for the intelligent layman and should review the main relationships between climatic conditions and food production; special attention was to be given to unfavourable climatic factors and to the analysis and forecasting of weather which is adverse to food production and also to the means of reducing the impact of unfavourable weather conditions. The pamphlet, written by L. P. Smith of the United Kingdom Meteorological Office who has now become a well known authority on all aspects of agricultural meteorology, has recently been published by WMO as Basic Study No. 1.

* *Weather and food* (Freedom from Hunger Campaign—Basic Study No. 1), by L. P. Smith. 9 in. x 6¼ in., pp. 80, *illus.*, World Meteorological Organization, Geneva, Switzerland, 1962. Price: *Sw. fr.* 2.

Before discussing this pamphlet however, it is appropriate to mention that the July 1961 issue of the *WMO Bulletin* was specially devoted to the theme of the Freedom from Hunger Campaign. The aims of the Campaign and the relevance of meteorology in food production were presented in an inspiring article by B. R. Sen, the Secretary-General of FAO. He particularly emphasized how valuable it would be if extended and long-range forecasts could be issued and gave some very interesting particulars regarding the world's agricultural resources; he mentioned, for example, that the 10 per cent of the world's land surface which is at present under cultivation by no means marks the limit of the land which could be cultivated by making full use of available water, by proper soil management and so on. In an article on "Weather and fisheries" T. Laevastu of the Fisheries Division of FAO gave some useful information on the influence of weather on the abundance and availability of fish and urged that forecasts for coastal fisherman should be more detailed, especially in regard to winds, and should be extended to cover 48 hours and longer periods. P. M. A. Bourke, the President of the WMO Commission for Agricultural Meteorology presented under the heading "Meteorology and mud" a review of international collaboration in agricultural meteorology, whilst C. I. H. Aspliden, a WMO technical assistance expert, and R. C. Rainey of the Anti-Locust Research Centre, London discussed the findings of a WMO Technical Assistance Mission in an article entitled "Desert locust control". It appears that the movement and distribution of desert locusts, on the scale of synoptic meteorology, are not merely correlated with, but are to a very large extent determined by, the corresponding low-level wind fields and the authors went so far as to suggest that, in areas from which meteorological data are scarce, reports of locust distribution and movement might assist the synoptic analyst to decide between alternative constructions of the wind field! The authors also put forward the view that the forecasters concerned should acquire the same kind of acquaintance with the relevant operating characteristics of locust swarms as they have of the aircraft they serve. In the same issue of the *WMO Bulletin* is another article, by L. P. Smith, giving a preview and summary of the booklet *Weather and food* recently issued by WMO as Basic Study No. 1.

The booklet consists of three parts. The first deals with "The influence of Nature". The meteorological parameters, length of day, heat and light, clouds and sunshine, rain, snow and temperature are treated separately. As Smith points out in his article in the *WMO Bulletin*, this is not really satisfactory because plants consider them together—but the manner in which they do so is complicated and intricate and has yet to be fully understood. Following the discussion of individual elements there are sections dealing with disasters, pests and diseases, soils and climatic classification. In regard to the latter, Smith points out that because the relation between weather and plants is complex, it is not possible to devise simple classifications and even complex classifications such as those based on potential evapotranspiration are of limited value. From his experience of this matter as a President of the WMO Commission for Climatology, the writer is inclined to support the view that, allowing for what can be achieved by means of irrigation, the use of glass and shelter, the natural vegetation itself provides the best classification of the climates of the world for agricultural purposes. In this connexion, it is curious that for some crops, such as wheat, the optimum rainfall appears to be very near

the irreducible minimum; thus, in a country with an average rainfall which is an optimum for a given crop but where irrigation is impracticable, a few years below average could mean starvation.

The second part of the booklet discusses "The influence of Man". In the first section, entitled "Plants and animals", Smith reiterates the fact that because of the uncertainty of what meteorological factors are important, it is difficult to compare one climate with another; hence the possibility of introducing new breeds or varieties from other lands is not a simple matter. He goes on to say, however, that "the more we learn about the logical practical process of climate classification the easier such methods will become, and in fact tremendous advances have been made by this means"—but it is not clear what these advances are. The sections which follow deal with erosion, irrigation, shelter, pests and diseases. Whilst paying tribute to man's accomplishments, Smith rightly condemns his misdeeds, especially in regard to soil erosion; but one wonders whether misuse of the land has not often been due to a selfish or "couldn't-care-less" attitude rather than to simple ignorance of physical factors. Surely, over the centuries, Man must have learned *from experience* the folly of over-cropping and the indiscriminate cutting-down of trees without having to be told of his sins by the scientists! In regard to irrigation, Smith rightly emphasizes that the control of water is the key to development and that the meteorologist is one of the experts who can help to fashion the key—but it would have been pleasing to have been given some examples substantiating his claim that in temperate countries where crops may grow without irrigation, yields have at times been doubled or even trebled by means of skilled irrigation. One wonders if this applies to *all* crops? In the next section on pests and diseases, the statement that "the relations between weather and pests and diseases are slowly being discovered" is a polite way of calling attention to the present ignorance on the subject and points to the need for much more research. In this connexion, one would like to know the real economic value of the warnings which are issued regarding the onset of potato blight. The remaining sections in Part II of the booklet deal with fires and floods (is it impracticable for us to have an inland flood warning service in this country?), glass and other material aids, frost, forecasts, and weather control. In regard to the last subject, it is pleasing to note that Smith does not raise wild hopes of the large-scale modification of climate as has been suggested in some quarters. Part III of the pamphlet relates to "The Future" and here, perhaps, the author is somewhat optimistic, especially in these days of subsidies, credit restrictions and tariffs. He calls for new thinking (on the part of scientists, administrators, farmers and farm-workers—especially "applied" scientists *working together as a team*), new facts (certainly there are gaps in available meteorological data which ought to be filled, but how much more data are we to acquire and never work up?), new experience (from field experiments), new knowledge (Smith emphasizes the value of making known research efforts which have failed as well as those which have succeeded) and new services. In regard to the latter, the author contended in his article in the *WMO Bulletin* that if we could serve the farmer as we serve the pilot crop production could increase tomorrow! He also emphasizes the need for extended and long-range forecasts and points out that an accuracy of two correct forecasts out of three could be of inestimable

value. In connexion with the forecasting of the incidence of pests and diseases his claim that successful forecasts have been made "for days, weeks and even *months* ahead" enabling preventative and curative methods to be applied efficiently is a little surprising. One would have liked to have been given some examples. Regarding the need for more education the writer whole-heartedly endorses the author's view that "the one who makes the most use of meteorology is often the one who knows most about meteorology". The weather-wise farmer or fisherman can certainly make the best use of the forecasts provided for him. The concluding sections relate to new lands (but including the sea for, as Smith points out, in most countries sea fishing is still at the equivalent stage in agriculture of harvesting wild plants grown by chance) and new responsibilities (involving international co-operation in general and the WMO Technical Commissions in particular).

The pamphlet concludes with the author's suggestions for further reading—amounting to over 60 publications! This indicates the need for a comprehensive textbook on the subject. Maybe, FAO and WMO can sponsor such a publication. Certainly, WMO is to be congratulated on Basic Study No. 1. The author has succeeded admirably in meeting the requirements of the Executive Committee. He has dealt with a complicated subject covering a very wide field in a clear and entertaining style and, knowing his enthisuasm, he can be forgiven if here and there he may have been a little starry-eyed. The booklet is well produced and illustrated and for two Swiss francs it is good value for money. All those who are interested in the subject should add the booklet to their library.

NOTES AND NEWS

The effect of wind on droplet-laden cobwebs

The photographs between pages 224–225 were taken at Dunstable on the morning of 31 August 1961 and show the effect of a gentle breeze on cobwebs heavily laden with droplets of water deposited during a fog. In Plate I, taken at 0705 GMT, the sun had just risen above the downs and was lighting up the web from the back; the drops can hardly be distinguished although the camera was only 13 inches away.

In Plate II, taken at about 0710 GMT, a gentle breeze was moving parts of the web, causing droplets to coalesce. Plate III was taken at about 0730 GMT and shows the same web as Plate II. The droplets had for the most part run to the intersections of the web, although in some parts, tiny droplets were still adhering to horizontal crosspieces.

Plate IV, taken at 0735 GMT, shows the droplets had all gone to the intersections and in some cases had formed quite large drops.

The Dunstable Meteorological Office observations, made about 1½ miles from the site of the photographs, gave a clear night with falling temperatures until 0600 GMT when visibility dropped to 1500 yards with 8/8 stratus at 100 feet; at 0700 GMT visibility was 150 yards, sky obscured, temperature 10°C. Soon after this observation, the fog lifted rapidly to give stratus patches.

R. K. PILSBURY

HONOURS

The following award was announced in the Birthday Honours List published on 9 June 1962:

C.B.E.

Dr. A. C. Best, O.B.E., D.Sc., Director of Services, Meteorological Office

AWARDS

We have received information that Mr. G. C. Sclare, Scientific Assistant at Cardington, and Mr. I. D. Cattermole, Scientific Assistant at Oakington, have been successful in reaching the Gold Standard of H.R.H. the Duke of Edinburgh's Award Scheme.

They both attended at Buckingham Palace on 15 June 1962 for presentation of their awards by His Royal Highness the Duke of Edinburgh.

OFFICIAL PUBLICATIONS

The following publications have recently been issued:

SCIENTIFIC PAPERS

No. 12—*Some statistical relationships between the temperature anomalies in neighbouring months in Europe and western Siberia*, by J. M. Craddock, M.A. and R. Ward

An estimate is made of the strength of the association between the departures from normal, during many years, of the mean temperature in different calendar months. This is carried out for each of about one hundred meteorological stations forming a network over Europe and western Siberia for all possible combinations of months up to a maximum separation of six months. It is found that strong evidence of association is almost confined to pairs of adjacent months, and to particular geographical areas which vary with the time of the year. The nature of the association is discussed.

No. 13—*Three-parameter numerical forecasts at Dunstable—a study of the error fields*, by C. E. Wallington, M.Sc.

Geographical distributions of errors in forecasts made with a three-parameter numerical model of the atmosphere show several distinctive features which may be used as an aid to interpreting the predictions. The distribution of errors in the numerical and conventional forecast have much in common. The neglect of topography, friction and possibly heating over the land in the model is evident, but the model is efficient at predicting broad-scale development and movement of pressure contour features over the sea some distance away from land masses and from the boundary of the computing area.

PUBLICATIONS RECEIVED

Philips technical review Vol. 21, No. 7—includes description of an automatic dewpoint hygrometer based on the Peltier effect. 11½in. x 8 in., pp. 185–220, N. V. Philips' Gloeilampenfabrieken, Eindhoven, Netherlands, 1960. Price: 3*s.* 4*d.*

An outline of the climate of Greece, E. G. Mariolopoulos. 9½ in. x 6¼ in., *illus.*, pp. 51, Meteorological Institute of the University of Athens, 1961.

Indicating and recording gauges. 11 in. x 8½ in., pp. 57, *illus.*, Negretti and Zambra Limited, 122 Regent Street, London, W.1. 1961.

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